Group Interview 4

VR: Talk a little about the research that was done. Yesterday you mentioned that the first grant largely concentrated on you doing your individual study sites using the same methods, and the second grant was the large study that was done on the Salmon river. Could you tell us a little about the research component of this and the number of papers that came out of this indicates that this was really a very strong part of the RCC. It wasn't just theory. It was actual hypothesis testing. So maybe we'll talk about the individual sites, and then concentrate more on the big Salmon river study. Anybody like to open up?

0:43

RV: Well on the White Clay Creek we selected three stations. A first order, second order, and third order site in a woodland setting, a small eight hundred acre…hectare site, and then to get larger streams, we had to move over to Doe Run, which is a large holding on the King Ranch in the Brandywine drainage system. Unfortunately the White Clay as it went downstream received too many impacts to be a natural system. So that was one variation in the system. It wasn't the same river system. But it seemed to work very well with an adjacent watershed. And I think the parameters that we measured during the primary production that Tom Bott Was instrumental in designing and working on that part of the project. Benthic activity, benthic organisms and the detrital component inputs and standing stock and transport. I think it was the standard methods that we all used across the biome, and at times we went to each other's site to see what was—how they were performing and what their sites looked like. So we had interactions during the course of the study. We all visited everyone's sites.

2:23

VR: How many years did this first phase go on for?

WM: Well there was the two year award . . . so two years and a renewal. So four years total.

VR: The renewal being the Salmon River?

WM: Yeah, yeah. Well and cleanup on some of the other sites, I think. And perhaps a lot of data processing and manuscript preparation in that period.

2:56

VR: Wayne, could you talk about the sites that you worked on?

WM: Yeah, our sites were on the Salmon river, although we spent some time just looking at several locations in Idaho, in Southern Idaho in particular that were accessible from Pocatello at Idaho State University. And we initially chose the Salmon for a couple of

reasons. One, because of the continuity in the system, it did go all the way from a first to an eighth-order system, and we thought that was quite attractive. And secondly, there was minimal human impact. There's a substantial Federal holdings, forest service, wilderness area, and other land, and relatively little development. There were a couple of towns, mostly small towns, Salmon Idaho being the largest, and that was way down in the lower part of the system. By the time, it had already attained an eighth order in size, or seventh order at least. So after that, our decision was to work with the upper regions so we would match pretty much what was going on at the other locations in terms of size. But we also tried to sort of push it a bit downstream to get it to the largest system that we could handle, and I think that was maybe comparable to the McKenzie river site in terms of its current velocities and depths and widths and things of that sort. And then other than that there was some more development of methodology that was specific to our sites, and one of our folks, Jim Brock, took the lead in devising some really innovative transport sampling techniques, and I think that those ultimately became sort of commonly used or at least it was a number of really innovative ideas that went into his approach, and were useful when we upscaled to a larger system as well. Other than that, I think we handled most of ours, because we did not have the depth in terms of other people. We did most of our actual sampling and development in this phase with graduate students. I had two supported directly as RA's, and a couple of others that were partially supported. And in particular, I think that as we mentioned earlier, Jim Brock's name keeps coming up, and he was a master student at the time, but well ahead of his time in terms of his abilities, and he made a tremendous contribution to the program.

5:36

VR: When you had these innovations in the methodology, how did you pass them on to the other group, or was it really intended to wait till the Salmon?

WM: No, not at all. I think there was a lot of phone things. Jim was a very outgoing sort of person, so a lot of this was passed on, probably by phone. But also, then, the visits, we would showcase those sorts of things. And I remember on several of these meetings where we would meet on a river bank, basically do a show and tell, as part of the operation. And in some cases test out the methods there, other cases set up a workshop that went after those in some detail.

6:17

BC: He was actually building the chambers.

KC: Right, yeah, that's right. He built them all.

JS: From those workshops you'd start an idea, and the methodology development was an intense interaction, and there was a normal amount of arguing, so there're a lot of parallel testing. We'd check it, and see what it was, and work it. And that was some of the most intense of discussions on how we're going to get an accurate measure, and what it'd take

to get an accurate measure of say primary production or respiration or a particulate organic matter.

BC: The detritus workshop, even though we finally settled out on a particular apparatus or device, it was more on the sizes, and how you got the right amount of water through it and got the sizes you wanted.

WM: Yeah, in fact up until that time, there was no routine wide-scale partitioning of particulate organic matter? This was really the first major effort. And it really has set the stage in what people have done. There might have been some refinements along the way, but for the most part, those size cateogries have held up and found to be functionally important and accurate.

7:44

KC: Those chambers made it out of the continuum project even then. Dick Marzalof's students on the Konza Prairie built chambers, with Jim's help built those, and those chambers were running in streams on the Konza about the same time we were doing the Salmon So this stuff, Cathy Tate And a bunch of people were actually using the same design, and those papers are all over the place now. And Jim's continued to—I mean, when he came to Florida, they were completely new designed, remotely operated valves, and all kinds of stuff. So he just never stops.

8:22

RV: Tom Bott's Chambers have been used this past summer in the Catskills and New York State by Tom and it's true. Measuring . . . This is a technology that developed, and has really continued.

8:42

VR: One of the things, before we go on, what were the years of these grants. Do you remember like the first two-year grant?

BC: We got notification that we got the grant in seventy-five.

JS: In the fall or something like that

BC: Just a second, let me get the . . .

KC: My sabbatical was seventy-six?

BC: July of seventy-five. We were meeting in Pocatello and had the grant in hand. So that was when we really had to . . .

KC: Do you remember what the amount was?

BC: No I don't. We could dig that up, but . . .

KC: It was big, but it wasn't huge. I mean, was it a million?

JS: No, no. I think it was a few hundred thousand, spread over course of . . .

9:41

VR: And then there was a Rockefeller grant that brought all of you together initially, correct?

RV: Only to provide funding for travel for thirty to forty people.

VR: And what year was that, Robin? Do you remember?

RV: I think it was fifty two or—no. Seventy-three or seventy-four. That was initially outside of the—

VR: That was a very important meeting that got . . . And then the second grant would have run from . . .

JS: Seventy . . . seven to eighty or something like that.

BC: Yeah, cause it was three for five years. The total is five. Two and three.

10:25

WM: And remember that there was a smaller grant ahead of that that provided for the working and the interaction. That was NSF. So we had sort of three NSF's. A small one that we don't really count, in preparation, and then the two major ones that were the river continuum project.

VR: And the second grant? That was the one that was about a million?

WM: It was just over, I think. As I recall it, the first one was around eight hundred thousand. The second was like one point two.

BC: I always thought we totaled pretty close to three million for the five years. That's the figure that always came to my mind, but maybe I'm wrong. The total for the whole RCC for five years was just short of three million.

11:16

RV: I think that what's always interesting, however the amount the money was, there was never a squabble over the partitioning of that money, and who needed people for

different projects. That flowed very smoothly. Everybody knew how much work it was going to be. And I think a lot of work was actually subsidized through other projects and other monies. And perhaps the major shortcoming in my mind on the funding part was that the project would have been much stronger if we had sampled more frequently during the year. This was sparse. And Wayne's crew had some very rigorous areas to get into at really difficult times of the year.

BC: I was going to say, frequency of sampling was a lot different in your shop then up there in the mountains cause it was—

RV: The logistics of Wayne's effort, and actually the Andrews didn't have an easy time.

12:28

VR: So let's hit some of the other sites. Jim, you want to mention the Andrews?

JS: Well the Andrews, we anchored that in the Andrews Experimental Forest. And again, our looking at choices, it was easy to find the first five orders, and then we chose the McKenzie which hydrologically is very different, but it's Lookout Creek Flows into Blue River flows into the McKenzie river. And so the McKenzie was our big site. And we had no clue how we were going to work that. Cause it's just a big honking river. But the others could play off of work that Fred Swanson, Frank Triska, Stan Gregory were working on. So I second what Robin was saying. We had a lot of subsidies. A lot of our technology, innovation, and the work, both on detrital respiration and that came from people. We weren't funded at all by the continuum. Stan Gregory And Frank Triska, we might have paid travel for them. But their contribution was over and above. And I don't know if we paid Bott a whole lot.

13:35

RV: Right, he was totally subsidized.

JS: So we had this network. I think everybody's group had folks that we drew in just for the excitement of not only the methods but trying to get that accomplished. So we chose to go with a post-doc , because the main crew at the Andrews were pretty tied up doing things for other NSF grants, and the IBP programs were kind of winding down, and so we got a post-doc lead and a new crew of technicians to do that. Some of them have gone on, done some pretty interesting things. Naiman was the post-doc lead Bob Naiman And then, Chuck Hawkins, Dale McCullough, Karen Lukessa we hired as technicians. And then we drew on people like George Lienkemper who worked with Fred Swanson And I think if we had something that we pioneered and did cause it was an extension of what we were doing in the Andrews, was measuring big wood, mapping it, and trying to look at how that might shape habitat and some of the features of the streams, that went off to the other sites. I know George was on the main Salmon as well as at the . . . But our approach was not unlike the rest of them, only we chose a post-doc lead

15:12

VR: Are there are any other sites that—

KC: We did the Kalamazoo all in the same basin. A first order and a third order site which is in the Kellogg forest, which is a part of Michigan State's holdings. So we'd been working there already. So that was one of our Fizz. So we did one, three, and five. The fifth order site was on the Kalamazoo above where the dams are around Battle Creek which is the Kellogg Company area. And what has Robin said, the subsidy, and Jim as well that all of us, everybody in my lab was involved in the continuum project. Including guys who were working on salary, something about a curiously strong mint…So everybody understood that when we were doing continuum work, everybody did it. And so the techs for a lot of my people were in zooplankton work at the time for NSF. And so it was just understood. And there was no complaint, because when they needed something, everybody—I mean that's just kind of the way it worked. And Roger Ovink was our guy that was doing the primary production in the continuum part, but Donna King was doing a thesis using her chamber for primary production, and she was looking at sites, little windshield-washer motor driven off a battery that was stirring the chambers. And so everybody had kind of their own thing, but when it coalesced into these beautiful chambers that Jim was working on, everybody kind of got on board with that. So it was definitely a subsidized team effort. It was just understood that this was the most important thing going on and everybody had to take part. I think that our contribution was because Mike Klug was there. I mean, he was really interested in the microbiological aspects, and I think he was the one that said there's something different microbially that happens at a particle-size of about a millimeter. When you get down to a millimeter, these fungal mats cannot maintain themselves on particles like that. And so the hyphomycete fungi were really in coarser stuff and then it became a bacterial show. And so the biochemical tracers they used showed that. But then he got us involved in using Gilsonn respirometry To track the respiration of different particle sizes. To see whether there was some signature that—well the sizes we decided on which I think were ecologically based and sound, was did the microbial stuff track that And it did. And we did a lot of experiments with . . . His technique was to kill the stuff with ethylene oxide And then see what happened without the microbes. And that was all kind of lab-based. And so Klug put a lot of time into continuum ideas, and he was totally supportive of the things. So I think without all this peripheral and subsidized, we never would have pulled this off. And Robin's exactly right. We needed more sites and more times, you know. So we can be proud of something. We did a heck of a lot with a kind of constricted resources.

18:53

WM: One place where we did not go, which I think I've always think that we've covered inadequately, was in the benthic production site. I mean the real further tests of the river continuum was to know what the invertebrates were doing. And we had to back off of that. We did not have the manpower, the people to work on it, we did not have the

funding. So it was both a conscious decision and a group decision to say we were gonna leave that untouched.

19:21

RV: Yeah, all the money could have gone into that one area to do an adequate job.

VR: Was there research being done at other labs that you were drawing into? Like for one thing Stu Fisher by then, was he in Arizona?

JS: Yeah, but he was en route. I mean, he was just leaving Amherst and setting up shop in Arizona State. But I think aside from—I mean we used a lot of Fisher's stuff at the Andrews. In fact, that's what when Frank Triska and the rest of us published that nitrogen budget. I was really trying to refine to our system and do just a budget. And we were really playing off Bear Brook. And we were obvious, and Stuart knew it. I mean, he helped us and whatnot. But when we got to the continuum, I quite frankly didn't see there was that much coming in from the outside. Most of it was generated inside. And that would people from the Konza and many of those who'd started or been associated with one of the other labs, and then they would take off on a different tack.

20:40

VR: You see, you're generating new research, rather than . . .

WM: And again, we were children of the IBP, so this whole energy budget focus and nutrient cycling focus was really that was an IBP high point, I think. But also, if you're studying eco-systems, that's where you go. I mean, those are ecosystem level parameters. And that's where you start to focus. So the rest of it was the embellishments, the petitioning, the whole new directions that were taken, I think came from with in the group. And what was exciting about it, I mean, that's where the sparks were. And it was, I mean the IBP on energy budgets was really a place in space and through time that, I mean you had a weird pullage or something. And certainly now you were doing something on a longitudinal gradient. And we didn't have a clue as to how to adequately do it. We knew we had to arbitrarily choose a spot to do it, and we knew that that wasn't quite what our vision was, and this is what got into our—there was this tug-of-war between the necessity and arbitrariness of coming up with the same measurements, and also of picking a site. And knowing that there's a lot of variability from first order to fifth order, and where do you choose that site, and how do you integrate things along there. So that that stimulated a lot of the excitement, I thought.

22:11

VR: Bert, you want to add anything?

BC: I was going to say, it's almost like arguing against what we were trying to prove. By picking arbitrary sites to prove a downstream thing.

VR: Let's talk a little about this second grant, the work on the Salmon Where you brought all your developments from the first studies into this one river system. Robin mentioned a little about why the Salmon was picked. Of course, Wayne, you had been working on it. Could you talk a little about that project?

22:39

WM: Yeah, I'll start maybe. But one of the things, the overall driver is, we're trying to get into the bigger systems. So we were trying to extend beyond where our individual locations had gone. And we felt the only way we could handle that operationally, logistically, was to marshal all our forces on that particular system, whatever it might be. And because of some of the things that were mentioned by others, where they ran into system changes, or in another watershed, or pollution all down, I think the Salmon came out clearly as the favorite place because of the lack of those other confounding issues. But that gave us a big system and one that, as has been mentioned earlier, is difficult to access, sort of our lowest site, our eighth-order site, down near Riggins, was near the other end of the state. We had to go all the way to Boise and up north just to get to that site, so it was a day's drive basically to get to the site to do it, and so we took it sequentially down the system rather than doing things simultaneously at all the sites, especially in that lower one, because of that logistical problem.

23:54

VR: Were you worried about seasonal differences?

WM: Well we had always been worried about seasonal differences, and we tried to do the four seasons, but when you get into that lower section and the logistics and so on, and then bringing people from across the country, where the, you know, land-based travel. Robin came all the way with trailers and sort of a wagon train of folks to get across there.

RV: Thirty-five people. Out there in March. Late March.

WM: So we looked at three seasons, basically, Early as we could do it, the summer, and then a fall if we could swing it.

24:28

VR: So you brought people in for weeks at a time each season.

JS: It was a three week pulse, essentially.

VR: Yeah, three times a year.

RV: Built a lab there and distributed the work station

KC: Remember Big Red that did it up at Oregon State? Awful pickup truck with a huge trailer on it—

JS: He bought a pickup truck to—

KC: Make fun of—and that truck ended up—the title ended up in my name somehow. I was the owner of that damn truck. First the damn truck, and then there's insurance, and oh God.

JS: It had a camper unit on it.

KC: Yeah, and owning it really—The camper was great. It was battery-powered, and all the chemistry inside and—

25:10

BC: Can I stop just a second? I think there's one significant event that we ought to talk a little bit about, and that was the Middle-Fork float trip. And that sort of came in before we started the big push downstream, wasn't it? It was in seventy-eight, and

WM: My remembrance of that is that we were doing the Salmon, the main Salmon, and we did the Middle Fork, as an aside kind of to explore some other questions that we had.

BC: But whatever it is, I don't want to miss that. I thought that was pretty important.

JS: Because I left the continuum—

WM: Well, can we come back? I think we ought to stay focused on the purpose.

JS: The main Salmon is really the second part of the River Continuum. The Middle Fork is a sideshow.

WM: So a couple of things that we need to mention here that are important as well, that this is when Bob Peterson Came out. So he'd been the coordinator and based at Stroud And he came out to Idaho and set up shop there and basically did a lot of the legwork to see what the sites were, we went out together and kind of selected the final locations. Chose a central location within the Salmon River Basin that we could rent housing and food producing facilities and a lab, and to get that set up, and then some of that was transmitted back, like George Spengler I know it came out with all of the benches and everything pre-fabbed and the hood pre-fabbed, and they just assembled it on site. So it was a major logistic kind of operation, in which Bob was playing an extremely important role in doing the coordination here. Also there was this whole deal with the land holders and owners and people like the forest service, and we had to coordinate with fish and game people so that they understood where we were going to be, and what was going on, and what the commotion on the river was, and they were clear on it. And that took a little

bit of public relations and outreach in terms of just—We made a brochure. I think it was floating around yesterday, that was to inform people what was going on.

27:22

RV: I think there was a newspaper article. Prepared the way and again, that was really—

KC: One logistic thing that shouldn't be glossed over was that when you're talking about the numbers of samples and things, the labeling issue. So that's the only thing that Spengler really—I mean he assembled a lab, but he also brought the labeling issue. And that became a real issue. Getting all these samples labeled, I mean that seems trivial, but by god, that's not trivial. I mean, just keeping track of—I remember he had these thousands of bottles.

27:59

JS: These report barcodes

KC: Barcodes sure would have helped.

RV: And we had another challenge to keep all these generators going twenty-four hours a day to run that PR chambers in the cold weather, or any time. And we fortunately had a fellow called Bill Shaw who was just one of these guys that could make any motor run and any electricity run through any wet circuit. To keep all these flow chambers going, you really had to have a mechanic somebody in charge of every detail.

BC: I've had more than one person ask me when I show a slide with all the chambers out in the Salmon and all these wires is, "How many people did you electrocute?"

28:47

WM: Why don't you tell them?

RV: Right, and somebody had to design a trailer to dry the wetsuits so they could be put back out without somebody being cold.

WM: Yeah, so a lot of the lower section or—maybe all of it was done, the invertebrate sample, was done with divers. And even the placement of the chambers. So we had a whole team that had been trained and had practiced in various such situations so that we felt comfortable with operating in that kind of—

29:21

KC: Talk about Scott, too.

BC: Yeah, Al Scott we brought. He was a trained diver in my lab up in Washington. And even though he wasn't associated with the continuum or anything, but we got him on to this diving, his expertise in diving.

RV: Oh, it saved people's lives to have him there, for sure.

BC: Yeah, he was real key to just integrating the whole diving program.

29:43

VR: Did you have any serious accidents during any of this?

JS: None that we'll talk about. No, we almost lost Bernie.

KC: Ruptured eardrum.

RV: Right. I went home for a fifth lumbar operation.

JS: Well see I remember one of those, on the lower river you got used to—and the diving was tricky if they were trying to hold on in a really fast current, and I know that we had one person had to just drop out his weight belt, and got tumbled over and bruised up a bit. But for the most part we didn't. But you'd start to be lulled into thinking you knew something about the river. And you'd look at a pool, and the current would be going down. And you knew if you got out about fifty feet, got down, the current'd be ripping, and you'd be in dead water. And so occasionally, you wouldn't start at the head of a pool or a run. You'd start in the middle or the lower third. Big mistake. And you'd get out and sometimes the run never got deep. You went through a whole air tank and panicked trying to get across the river before you hit the rapids below. And there were a lot of things that was trial and error learning.

31:09

WM: Ominously, one of our sites was called Dead Man's Hole. And it was maybe the first one down that we started working with so it was shaky on occasion just for that.

VR: Did the research go as you predicted? Were there things that just constantly came up that you had to do modifications?

BC: Oh, you don't do anything at these sites without running into little things.

31:34

JS: I think the biggest thing that we were . . . We knew there'd be an issue, but it became a big issue, was just people. People issues. And it's integrating four, five crews who had been working very intensely with each other, and had worked out a rhythm, and how to accommodate people's strengths and weaknesses, and then suddenly you're jammed for a

very intense thing where you're going to run sleepless for three weeks, and trying to get that going. I think that was a huge issue. And certainly in our crew, we basically left our lead person back home to write papers on our site, because it was not going to work to have that individual interacting and trying to constructively make the bigger picture go. More on the PI's. There was some flare-ups and so on which we got by, but still that's a real triumph.

BC: I still have one slide of all five PI's working at the same table picking bugs, which is a rare slide. But it proved that we could do it.

32:47

JS: No, it was the people issues, and how we tried to accommodate that.

BC: An example of that, we had a little spat between the divers and the bug pickers. The divers go do their thing. The bug pickers then have to work into the night. And they got a little unhappy to see the divers out there tossing horse shoes. But these things worked out.

33:07

VR: Did the results live up to your expectations?

WM: Yeah, I guess, with the missing macroinvertebrates stuff, it just leaves a big gap in terms of interpretation. I thought, although it's long in coming, that our eighth order paper was a very good, complete analysis of what we set out to do. There's obviously lots of things we could do differently. A lot of understanding that's going on now that we could have applied if we'd had that background, but given that this was a pioneering effort, I think it worked out pretty well. We didn't talk a little earlier . . . For me, the sum of it turned out to be more complex geomorphologically than what I had originally anticipated. So we sort of did not go through the kind of classic stream changes, in terms of canyon, meandering braiding kinds of sections. And below Salmon, we get back into a canyon constrained section for a very long stretch. So just dealing with that conceptually and trying to sort of handle that in terms of what we originally set out to test. That added some challenges to it.

BC: I think the very fact that we're sitting down and talking like this indicates what it did. We hadn't have been pretty successful in putting this paradigm out, we wouldn't be sitting here recording it.

34:34

WM: Well, yeah, and not to sagehook, I mean, what I stated earlier. There are a lot of critics out there that can take shots, and I think some of those are well-deserved. There are gaps. It's not a fully documented test. And yet it may be the best shot that's ever

able to be marshaled, given the funding environment, and the changes in direction that some of the research is taking now.

BC: Likewise I'm sure there'll be people in the future that watch this tape and say, "Why didn't Resh ask this question?"

35:08

JS: I mean you'd think after twenty-five years, the field should be staffed with smarter people than we are, and that we'd learn something in twenty-five years, and yeah, they'd make huge changes or calibrate it to these different parts of reaches of stream or said something. I mean, if we haven't, then man, they're not doing their job.

35:35

WM: Well and you know, you'd like to think that budgets would accommodate things. Just take an ideas that you're going to do research very well. Multiply it by four, because that's the number of sites we were handling in a given year, and then multiply that by three to four, because that's the number of sites we were doing across the nation. You've suddenly have magnified the budget so that it's pushing the limits. Now if you want to add, which I think we would have done if we could have done it, instead of looking at transects, we would've looked at up-down sections. And we would double your budget right there. And then if you add the hyporeos into that, which we did not appreciate at the time, but which we now do, you'd add another component that had to be covered. And suddenly you're really pushing the limits of physical and financial ability to do some things.

36:18

KC: And then if you want to deal with floodplain issues, you have to have a river with a floodplain. So the Wilamette, or something, would have been—and of course it got isolated from its floodplain a hundred years ago, and where're you going to find the site to do this stuff?

36:38

VR: Well I guess the next question that I would think of is, once this grant was over, did the result influence other research that each of you did. In other words, did you stay on questions of the continuum that had come up say in the Salmon river study? Did any of your research go directly in that?

BC: Yeah, I think one of the main spin-offs, at least for me and for Wayne was the one problem and question that we generated with all this other work, was downstream transport defines some things. So after the continuum was over, Wayne and I pursued this with the Carbon-14 tracer stuff.

37:15

WM: Which was also an NSF funded grant.

BC: Yeah, another NSF.

WM: And the real key issue here is that if there's a continuum or progression, then the particle that starts in the headwater has to make it all the way through the system. And there was no way of verifying that, and the alternative would be that it's totally metabolized within a very short reach of stream, especially if you have a high retention. And what we're showing is kilometer levels of movement in a day's time. And easily projected, that could clear the system in the course of a year or less. So it's crucial to validating some aspects of the continuum that we couldn't pursue.

BC: And just as an example of how little we knew about this, when we were writing the grant, and trying to make up our own mind what length to look at this, we called a whole bunch of guys and say, "How far do you think a particle under these conditions would go" And the guesses were everything from three meters to three kilometers. That was everybody's impression.

38:17

KC: Then there was the coarse part of that, where we, based on some things that Robin had said early on, we now as a standard thing, use ginkgo leaves to track litter retention. And we do it in our classes all the time. And that resolves at hundred-meter reaches. You know, leaves, they don't go kilometers. They go meters. So there's a thing that happens between coarse stuff and fine stuff. So that's just kind of for us become a standard thing to calibrate different reaches for their retentions for the litter part. And ginkgo is great. They all come down at once, they're all yellow, and nothing eats them for a month. And they stay yellow in the water for months, so wonderful tracers.

39:01

WM: I mentioned earlier that particle size differentiation has just pretty well stuck as a standard into the dynamics now, and then although it started, I think with Ken's shop, it was integrated into the river continuum, and is really now very common that the litter decomposition efforts on putting packs of leaves out, looking at the rates of decay, and using that to sort of document microbial activity. And then Jim had mentioned earlier, the wood. That's been a big issue. That was a whole paradigm change, as we talked about yesterday, and just the very fact that people are documenting, keeping track of that, and considering that in terms of retention, which in the meantime, the whole spiraling concept evolved. We were able to apply that and test that with our data in the eighth order, cause we were set up to do that, and what's absolutely remarkable is that those predictions made with a very crude model at that time, now are being validated by the latest publication that Denis Newbold is senior author on with the elegant models that we've developed in relation to the FPOM transport.

40:15

VR: The spiraling idea came about independently, though, didn't it?

JS: It came really out of Walker Branch at Oakridge

WM: Independent, but we felt that it rode on the ground cut by the river continuum. I mean, spiraling would not have even been germane if there hadn't been the idea of continuity. If we had been thinking conceptually in terms of segments or zones in the old European fashion, there would have been no spiraling concept.

40:48

JS: And then it also stemmed, again, out of the IBP, where they were into nutrient cycling, and how do you translate that into streams, which don't at a point cycle.